

## **APPENDIX B.**

### **Peer Review**

## **APPENDIX B1**

### **LIST OF EXPERTS SOLICITED FOR PEER REVIEW**

<b>Name</b>	<b>Title/Organization</b>
Walt Duffy, Ph.D.	Unit Leader, Fish and Wildlife Cooperative Unit, Arcata
Michael Healey, Ph.D.	Westwater Research Center; Professor, Dept. of Oceanography, University of British Columbia
Peter Moyle, Ph.D.	Professor, Dept. of Wildlife, Fish , and Conservation Biology, University of California, Davis
Gordon Reeves, Ph.D.	U.S. Forest Service, Pacific Northwest Research Station, Corvallis, Oregon
Terry Roelofs, Ph.D.	Professor, Dept. of Fisheries, Humboldt State University
William Trush, Ph.D.	Director, Institute of River Ecosystems; Adjunct Professor, Dept. of Fisheries, Humboldt State University
Hiram Li, Ph.D.	Professor, Dept. of Fisheries, Oregon State University.
Staff	National Marine Fisheries Service, Southwest Region Fisheries Science Center, Santa Cruz.

## APPENDIX B2

### Peer Review Comments Received

**note: some of the comments below have been incorporated into the Status Review Report.**

#### National Marine Fisheries Service, Southwest Region Fisheries Science Center

April 17, 2002

Dennis McEwan  
California Department of Fish and Game  
1807 13<sup>th</sup> Street, Suite 104  
Sacramento, CA 95814

Re: NMFS SWFSC Comments on California State *Status Review of California Coho Salmon*

Dear Mr. McEwan:

Below, please find comments made by staff of the NMFS Southwest Fisheries Science Center (SWFSC) regarding the draft document “*Status Review of California Coho Salmon (Oncorhynchus kisutch)*” prepared by the California Department of Fish and Game (CDFG). Scientists participating in this review included Dr. Brian Spence, Dr. Peter Adams, Dr. Eric Bjorkstedt, Dr. Carlos Garza, and Thomas Williams from the Science Center, and Dr. George Boehlert from NMFS Pacific Fisheries Environmental Laboratory, who reviewed the section in Chapter VI on oceanic conditions. We appreciate the opportunity to participate in this review and hope that you will find the comments useful in preparing your final report.

Overall, NMFS SWFSC reviewers found the report to constitute a reasonably thorough review of the status of coho salmon in California. It is clear that CDFG went to considerable effort in compiling recent and historical information on the distribution and abundance of coho salmon in the state, and CDFG is to be commended for the extensive sampling effort it undertook in summer of 2001 to collect up-to-date information on the distribution of coho salmon. We know of no additional major available data sources that would appreciably alter any of the conclusions reached in the document regarding the status of coho salmon. Furthermore, we agree with the major conclusions of the report: that coho salmon in the Central California Coast ESU (north of San Francisco Bay) are currently in danger of extinction, and that coho salmon in the California portion of the Southern-Oregon Northern California Coast ESU are likely to become endangered within the foreseeable future.

There is one area in which conclusions of CDFG are inconsistent with those reached in the NMFS (2001) status review of coho salmon; this discrepancy relates to whether there is evidence that reductions in the distribution and abundance of salmon in the Southern Oregon-Northern California Coast ESU have continued into the 1990s or whether the losses occurred prior to the mid-1980s. This discrepancy (elaborated below) results from differences in analytical approaches, specifically, with respect to the temporal frames within which presence-absence data are aggregated. Fundamentally, these differences do not affect the ultimate assessments of

extinction risk. However, accepting the NMFS analysis would strengthen the CDFG conclusions that SONC coho salmon are threatened with extinction.

Aside from this, most of our remaining comments are directed toward improving the thoroughness, accuracy, and readability of the report. In the attached pages are chapter-by-chapter comments for chapters I through VI, and VIII through XI. The numbers correspond to numbers in the margins of the hard-copy text, which will be sent to you via express mail tomorrow. There are additional comments of a relatively minor nature written directly on the manuscript.

Again, we hope these comments are helpful in revising this important document. Please do not hesitate to contact me if you have any questions or need any clarification regarding any of the comments contained herein.

Sincerely,

Brian C. Spence, Ph.D.  
NMFS Southwest Fisheries Science Center  
110 Shaffer Road  
Santa Cruz, CA 95060

## Chapter I. Executive Summary

1. Pg. 1. Since the 1940 estimate cited on pg. 65 is a range (250,000 to 500,000) then the percentage should also be a range (6-12%). It would not hurt to also mention the estimated numbers for each of the time periods discussed.
2. Pg 2. Also, although this is an executive summary, you still need to provide sufficient information on methods so that these results can be interpreted appropriately. The reader has, at this point, no knowledge of what the “analysis by brood year” entailed, or what baseline (i.e., Brown and Moyle) was used to make this comparison. Without that information, these four paragraphs are difficult to understand.

Also, the 61% number does not jive with the number listed for 1995-2000 brood years cited in Table 5, pg. 54, which is 50%.

The statement that “*there does not appear to have been a significant decline in distribution between the late 1980s and the present*” disagrees with analysis of presence-absence data with annual resolution that indicates a decline in detectability in northern California (NMFS 2001). Based on this analysis, we reached a different conclusion: that the California portion of the SONC is not stable through the 1989-2000 period. Some attempt to reconcile these different conclusions is warranted. See comments on Chapter V for further elaboration including discussion of potential biases associated with the CDFG analysis.

Additional confusion results from the statement that “*The 2001 presence survey data **also** [emphasis added] show a decline in reported distribution in this ESU,*” which seems to contradict the aforementioned conclusion. Again, it is not clear what the benchmark or reference point is.

## Chapter II. Introduction

1. Pages 5-7. Section generally looks good. Only minor clarification needed (see marginal comments).
2. Pg. 7. It is worth noting that the conclusion of the BRT that the CCC ESU is in danger of extinction is not reflected in the listing decision, which gave CCC coho “threatened” federal status.

## Chapter III. Biology.

- 1 Pg. 10. Snyder (1912) does not list the Pajaro River as a historical coho stream. Also, although there are recent reports of adult coho occurring in Aptos Creek, Waddell Creek appear to be the southern-most stream containing persistent populations at this time.
- 2 Pg. 13. In general, the section on taxonomy and systematics looks good, with the exception noted below.
- 3 Page 15. It is not sufficiently clear in Table 2 that almost all of the California population samples in the Weitkamp et al. 1995 document are not new data, but only data from the other cited sources.

4. Page 22. Need citations to support summer and winter habitat requirements of coho salmon. Should also probably include some discussion of the fact that current models of rearing capacity being used in Oregon (Nickelson and Lawson), which place great emphasis on winter rearing habitat, may not be “exportable” to California, where summer low flows and summer temperatures may be important population regulation mechanisms.

#### Chapter IV. Habitat Necessary for Survival

1. Page 25. Many sections in this chapter need greater acknowledgement of sources. Some, but not all, of these are noted in the margins.
2. Page 26. Discussion of effects of temperature on embryos and alevins should include more than “optimal” and “lethal” temperatures. Because of the tight coupling of temperature and developmental processes, changes in thermal regime, even when well within the physiological tolerable range for the species, can have significant effects on development time (and hence emergence timing), as well as on the size of emerging fry.
3. Page 26. Low DO can also affect size and condition of emerging fry.
4. Page 28. This sections needs discussion of importance of linkages between sediment inputs, substrate quality, and potential effects on salmonids. Food production and cover in the form of substrate interstices are important aspects of habitat that are adversely affected by high sediment loads. These are probably bigger issues than turbidity in most cases.
5. Page 28. Spence (1995; cited in preceding paragraph) also found that the probability that coho smolts will migrate downstream increases with rapid increases in temperature.
6. Page 29-32. Be careful here with the discussion of “optimal” conditions. Most studies define optimal conditions on the basis of physiological responses or efficiencies under laboratory conditions. If one believes that coho salmon populations become locally adapted to the particular suite of environmental conditions in their natal stream, then “ecologically optimal” conditions may fall outside of the narrow range deemed “physiologically optimal.” Most important of these potential influences is the alteration in timing of life-history events. There is evidence, for example, that development time of embryos varies among populations, the assumption being that natural selection has operated to ensure emergence occurs at a favorable time. Similarly, smolt outmigration timing (and age at smolting, if you move farther north) has presumably evolved to ensure ocean entry at appropriate times. Consequently, small changes in temperature can disrupt the natural synchrony of biological cycles. Thus, in table 4, just as you indicated that the appropriate flow regime is specific to a watershed, so too is temperature regime.

#### Chapter V.

- 1a. Pages 35-36. The treatment of population structure is a bit superficial and could go a bit deeper into the primary literature, much but not all of which is cited in McElhany et al. (2000). That said, given that there is no attempt to link the data to these concepts in a quantitative way, an in-depth review is not really required. Are there reviews in the literature that might serve as a broader set of references than what appears to be sole reliance on McElhany et al. (2000) for context? A focus on hierarchical structuring in

salmonid populations and ESUs will likely prove more useful in placing the available data in context than will invoking metapopulation theory.

1. Page 36. Should also mention that, over longer periods, the relationship between source and sink populations may change (i.e., sources may become sinks and vice versa). Thus, protecting only “current” source populations may be inadequate to ensure long-term persistence.
2. Page 36. If you are going to suggest that hatchery and wild populations may function as sources and sinks, respectively, then you need to point out that 1) the reverse may also be true...wild “source” populations can be mined for broodstock. (Despite the release of thousands of smolts notwithstanding, survival through the entire life cycle may be inadequate to replace the wild fish taken from the population [see e.g., Currens 1995]); and 2) genetic effects resulting from hatchery-wild matings may change the relative productivity of wild populations, such that wild sink populations become even less productive through time (an important consideration given that millions of hatchery juveniles have been pumped into streams over the last 20-30 years).
3. Page 36. Connections over time are also potentially important for local population persistence. Gene flow between brood lineages in the same location may be limited, barring sufficient numbers of jacks or 2+ smolts in a population.
4. Page 36. A brief explanation needs to be offered as to why the status of salmon is being considered separately for the two ESUs. In that regard, the relationship between the populations from S. F. Bay to Punta Gorda and those south of S. F. Bay needs to be addressed. What are the implications of stocks south of S. F. Bay for ESUs, risk analyses, application of Brown and Moyle data, etc.?
5. Page 36. Need to note the distinction between population viability and ESU viability. With respect to the specific ESUs in question, it appears likely that there will be multiple independent populations within each ESU, and that ESU viability will require ensuring that a number of independent populations will need to be viable for the ESU to be considered at negligible risk of extinction.
6. Page 37. Mention of the Higgins et al. 1992 review for the Humboldt Chapter of AFS would seem warranted.
- 6b. Page 37. A comment on the assembly and analysis of data in the “Presence by Brood Year Investigation” section: the practice of assigning presence to a mainstem based on presence in a tributary potentially introduces a bias in identifying patterns and trends in presence-absence because the reverse mapping (mainstem implies tributary) is not applied—admittedly it cannot be applied. If the data are treated at an aggregate level with mainstems (and associated tributaries) as the unit, this may no longer be a concern; however, it is not clear whether this (necessary) rule was applied. Also, more information is needed on the application of the statistical tests to these data on p.53—perhaps provide the actual tables analyzed? Application of chi-square tests to proportion data is not recommended, and it isn’t clear whether this was done here.

7. Page 38. The narrative describing historical and current distributions (pages 38 through 52) needs significant reworking to 1) clarify the primary intent of the section, 2) provide succinct definitions of what CDFG considers “historical” and “current,” 3) provide greater consistency (geographically speaking) in level of detail, and 4) simplify the presentation.

Intent. It is unclear to the reader whether the overall purpose of this section is to 1) comprehensively review all available data on historical and current distributions, or 2) provide a direct comparison to Brown and Moyle (1991). This confusion stems, in part, from the fact that both the level of detail and the attempts to compare current to historical distributions (and to use Brown and Moyle as the basis for comparison) differ among watersheds. You need to decide whether the goal of this section is to provide a comprehensive view (defining your own rules and ignoring, for the moment, the Brown and Moyle list), or whether you want this to focus on a direct comparison between Brown and Moyle. Because the “Presence by Brood Year” analysis provides a relatively direct comparison to Brown and Moyle, we would favor the comprehensive approach in the narratives (though see comments on simplification below).

Definitions. To help clarify the intent of the section, begin with a clear statement of what CDFG considers to be “historical” and “current” occurrence. For all watersheds in Del Norte County, it appeared that you were treating any observation prior to brood year 1995 as “historical” and any subsequent observation (brood years 1995 through 2000) as “current.” For the Klamath, however, that structure broke down, and the narrative consisted of descriptions of various surveys, some of which yielded opposite results within the last 6 years (e.g., Kier indicating “presence” in the E. Fk. N. Fk. Trinity River in their 1999 report but CDFG indicating “absence” in 2001). The overall picture becomes murky with the details of specific surveys. Further, streams are omitted from the list of “historical” streams but then are cited later as “historical” in the description of current distributions (Bluff and Slate Creeks are two examples on pg 42; there are others as well).

We recommend adopting some simple rules (i.e., current means any time during the last two brood lineage cycles, for example), making those rules very explicit, and then summarizing the information according to those rules. The details of individual surveys are best left to an appendix list. What would also be helpful in the narrative would be greater emphasis on putting the results into a larger context. For example, at the conclusion of the Smith River section, you note that there have been few recent observations of coho salmon in two major subbasins (South and Middle Forks); this is an important observation given that the Smith River is often considered to be a “stronghold” for salmonids. In other sections, the summaries list streams without much geographic reference or simply summarize that fraction of streams for which recent coho salmon observations have been made; thus whether apparent reductions in distribution are widely scattered versus concentrated in a few subwatersheds is not evident.

Level of detail. As noted above, there is significant disparity between the level of detail presented for the various watersheds or geographic regions. For example, the discussion of historical and current distributions in the Smith River is very detailed, naming virtually every major and minor tributary for which there exists some record of coho salmon occurrence. In contrast, for the sections covering Mendocino and Sonoma County



watersheds, the information presented is of a summary nature, e.g., “*Recent status reviews place the number of streams historically containing coho salmon in the Ten Mile River watershed at between eight and 18*”); few specific tributaries are mentioned by name as either historical or current coho salmon streams. Because of this, it is difficult to get any sort of spatial picture from the data provided.

*Simplification.* Having said the above, we also wonder if presentation of this information couldn’t be substantially simplified with the use of tables. While the narrative approach does parallel the effort of Brown and Moyle, tables that list tributaries according to subwatersheds (along with presence/absence data) would provide an easy way to present the data in their entirety. The narrative could then focus on summarizing what the tables say, comparisons with Brown and Moyle, and overall spatial patterns, rather than get bogged down in lists of stream names, most of which will have little meaning for many readers.

8. Page 38. The use of the term “cohort” at various places in the document adds the potential for confusion—it is sometimes unclear if, say, the 1996 “cohort” is intended to mean the 1995 brood year or the 1996 brood year. Sticking to “brood year” reduces this ambiguity (though be sure to address the fact that spawning runs may span two calendar years, in which case adopting a convention of referring to 1995/1996 spawners as representing the 1995 brood year is warranted).
9. Page 40. Given that this section is focused on distributions, it might be more logical to put information on historical *abundance* in the subsequent section.
10. Page 40. There are a significant number of streams in the Klamath-Trinity system that were identified as historical coho salmon streams that are not mentioned in the narrative as such (see list in NMFS 2001a). NMFS pulled some of these data from Brownell et al. (1999), which in turn cited various USFS districts or other sources. Since Brownell et al. (1999) is listed as a primary source for historic and current information, what was the basis for excluding these streams from the narrative? If, in fact, you’ve done additional research that suggests these observations are either in error or impossible to confirm, then this would be important information to include in the document (perhaps as an appendix), since the work of Brownell et al. (1999) is often cited as a definitive source.
11. Page 54. Does CDFG intend to fill in Table 5? The data necessary to create such a table are available in appendices to NMFS 2001a combined with the CDFG 2001 surveys.
12. Page 53. Conclusions reached in this section regarding trends in the probability of detection of SONC coho differ from those of NMFS (2001) and subsequent analyses we have performed. Our analyses, which retain the annual resolution of the data, suggest that in the California portion of the SONC, the probability of detecting coho salmon is indeed declining over the period 1989-2000, whereas CDFG concludes that the declines between 1986-1991 and 1995-2000 are insignificant.

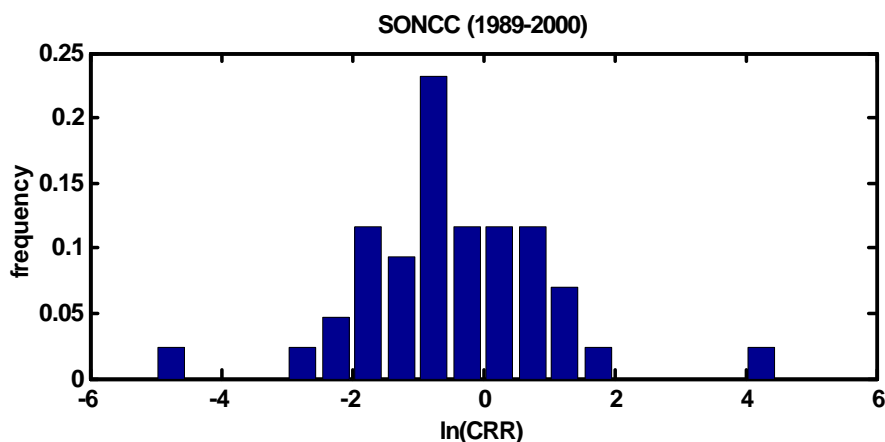
Analysis of presence-absence data pooled over brood-year-lineages is biased towards reporting presence—the loss of a brood-year-lineage in a basin cannot be captured by the aggregated data (i.e., one detected presence over a six-year period is considered equivalent to six detections over a six-year period when data are aggregated). The CDFG

data for 2001 presented in this report, which indicate substantially lower detection percentages than estimated from the aggregated data, suggests that brood lineages may, in fact, be disappearing.

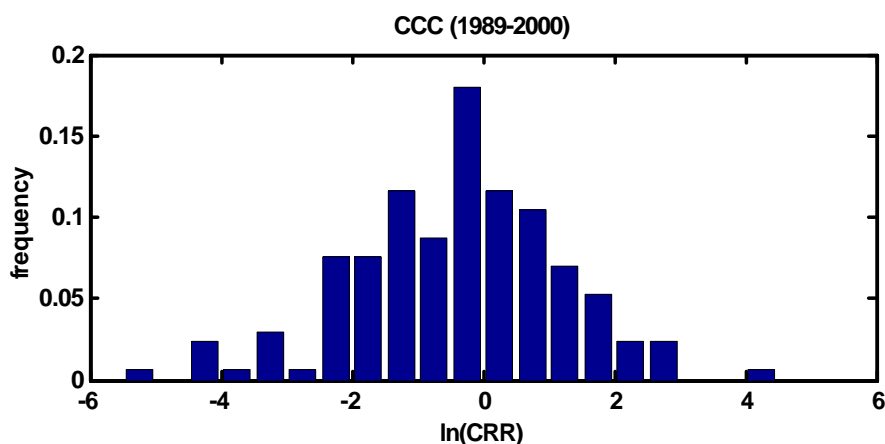
Note also that a second potential source of bias may be introduced if sampling effort (i.e., frequency with which a stream is visited during the six-year period) differs between the two sampling periods. There is some suggestion in the CCC ESU data that the probability of detection is increasing over time, but this is strongly confounded with temporal trends of increasing sampling effort over time. In analyses that incorporate weights based on sampling effort, the indication of increased detection in the south parallels the increase in sampling effort. However, a substantial change in sampling effort is not apparent in the north, which suggests that the apparent decline is less likely to represent an artifact of sampling in the data.

To summarize, our most recent analyses suggest the following conclusions:

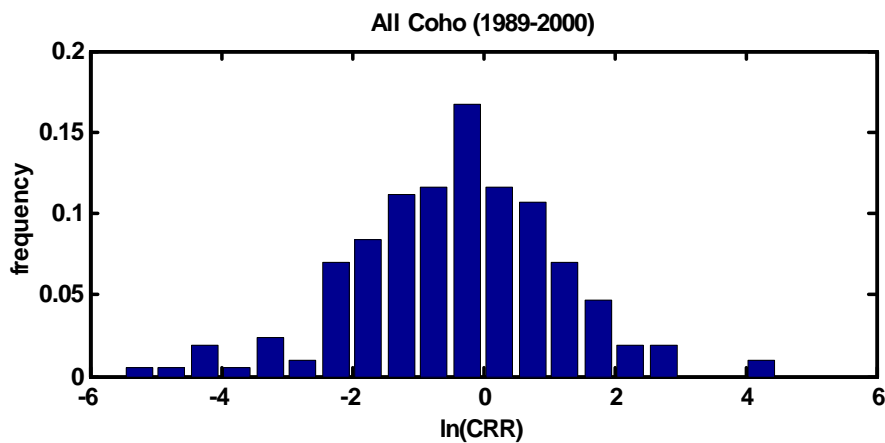
- coho presence (detectability) is declining in the north
  - coho presence (detectability) is lower in the south than in the north
  - trends in coho presence (detectability) are unclear in the south—apparent trends are confounded by substantial changes in sampling effort over the period 1989-2000.
13. Page 53. In the presence-absence analysis (by brood year), why was no attempt made to use data from the period between 1992 and 1994?
- 13a. Page 55 and pages 58-64. These figures showing spatial patterns are critically important as they highlight the fact that, in certain watersheds, coho were absent from large subbasins in several watersheds. Greater discussion is needed to bring more attention to these patterns.
14. Page 68. A new, more appropriate analysis of the CRRs has been done for juvenile and smolt data, which suggests that the mean  $\ln(\text{CRR})$  is significantly less than 0 (i.e., mean  $\text{CRR} < 1$ ) throughout California, as well as for each ESU considered separately (See plots on following page).



Analysis of Cohort Replacement Rate ( $CRR = n_{t+3}/n_t$ ) for coho salmon in the California part of the Southern Oregon-Northern California ESU based on paired juvenile or smolt abundance indices within the period 1989-2000. Mean  $\ln(CRR)$  (and 95% CI) is  $-0.4437$  ( $-0.8953$ ,  $0.0080$ );  $p = 0.0270$  for t-test of  $H_0$ : mean  $\ln(CRR) = 0$ .



Analysis of Cohort Replacement Rate ( $CRR = n_{t+3}/n_t$ ) for coho salmon in the Central California Coast ESU based on paired juvenile or smolt abundance indices for the period 1989-2000. Mean  $\ln(CRR)$  (and 95% CI) is  $-0.3869$  ( $-0.5793$ ,  $-0.1946$ );  $p = 0.0010$  for t-test of  $H_0$ : mean  $\ln(CRR) = 0$ .



Analysis of Cohort Replacement Rate ( $CRR = n_{t+3}/n_t$ ) for coho salmon throughout California based on paired juvenile or smolt abundance indices within the period 1989-2000. Mean  $\ln(CRR)$  (and 95% confidence intervals) is  $-0.3914$  ( $-0.6011$ ,  $-0.1816$ );  $p < 0.0005$  for t-test of  $H_0$ : mean  $\ln(CRR) = 0$ .

Note that these estimates are calculated from available recent data for juveniles and smolts. Inclusion of historical data, including adult data, exacerbates this result by incorporating the massive declines through the 1960s-1980s. Note also that t-tests are more appropriate than the binomial test used in NMFS (2001) for evaluating whether these results indicate a mean  $\ln(\text{CRR})$  that differs significantly from zero. In all cases, the null hypothesis that mean  $\ln(\text{CRR})$  is zero is rejected.

15. Page 69. Some discussion of the Sweasy Dam counts (Mad River) would seem warranted here. (These counts are mentioned in the conclusions, but all data should be laid out before the concluding section).
16. Page 74. We're not sure of the value of fitting the decomposition time series models to the smolt and juvenile time series, and plotting predictions of future abundance. Simply plotting the data with a fitted line (or three, to capture similarities or differences in lineages) would make the point adequately.
17. The decline in occupancy in the north is partly offset by the lack of significance in CRR analysis. The argument that presence of coho in watersheds, albeit at lower abundance and reduced distribution, represents a lesser fragmentation than to the south is not fully supported, though, as the underlying analysis is confounded by differences in watershed size. (There are, for example, significant portions of the Smith, Klamath-Trinity, Eel, and Mattole Rivers where coho were absent or vary scarce in 2001.) It might be, indeed is likely, that fragmentation observed in the north is obscured by the tendency to aggregate data for larger watersheds but not for smaller watersheds. There is perhaps some biological basis for this aggregation, but this depends on untested assumptions about straying within a large watershed versus straying among smaller coastal basins. Precautionary approaches would suggest considering whether fragmentation truly is reduced in larger basins as the consequences of such fragmentation are potentially disastrous.
18. Page 78. Analysis of the CCC seems incomplete, although evidence for declines in abundance based on CRR seems stronger than for the SONC. A greater and more comprehensive consideration of presence data seems necessary, if only to highlight the relatively poor quality of these data and lack of information available.

#### Chapter VI. Factors Affecting the Ability to Survive and Reproduce.

1. Pages 80-81. There some important shortcomings in section dealing with ocean conditions. These shortcomings are outlined below (in no particular order), and we have also included a list of additional references that the authors should consider and cite in the text.

The discussion in this section is heavily weighted by consideration of the El Niño Southern Oscillation (ENSO). While it seems likely that these events influence production of California coho salmon, there are certainly other climate cycles (generally at longer time scales) that influence coho production. There are hints in the text that the authors are cognizant of this fact, but the treatment is not balanced. We suggest that the authors reduce the emphasis on the ENSO and at least include discussion of the Pacific Decadal Oscillation (see Mantua et al. 1997).

The authors have described the physics of both the ENSO and wind driven upwelling incorrectly. The ENSO is not "caused by the weakening of equatorial westerly trade wind

patterns.” The physics of the ENSO are complex and, in fact, the trade winds are easterlies. Similarly, the authors state that “increased thermocline depth and stratification caused by the rise in temperature results in less wind-driven upwelling,” but winds, not temperatures, increase or decrease upwelling. (Upwelling continues, but the effectiveness of it in bringing nutrient-rich water to the surface is diminished because of increasing depth of the thermocline.) We suggest that the authors remove discussion of the physical mechanisms driving the various climate processes that affect coho production. The authors should, instead, simply state that these processes are physically complex and affect production from the bottom up. There are several recent references they could benefit from reviewing, including Cole (2000), Ryding and Skalski (1999), Hobday and Boehlert (2001), and Koslow et al. (2002). All deal with coho survival and ocean variability.

We agree that salmon are well suited to coping with environmental variation, but we do not agree with the authors’ opinion that “healthy and stable salmon populations... are generally not at high risk of extinction due to environmental variation.” There were certainly “natural” expansions and contractions of the range of coho salmon prior to anthropogenic influence. It seems plausible that these expansions and contractions (manifested by local extinctions and recolonizations) occurred because California is at the edge of the range of coho salmon. In fact, throughout the document there is insufficient attention paid to how ocean variability affects coho salmon in different parts of their range, particularly given that California is on the southern end of the range. See, for example, Hare et al. (1999). Also note that the plankton reduction noted by Roemmich and McGowan (1995) generally occurs at latitudes considerably south of where coho salmon occur.

The last sentence of the Discussion section states that “if the local extinction rate is less than the colonization rate, then overall declines will be observed.” This does not make sense to us. Perhaps this is merely a typo, and overall declines will be observed if the local extinction rate is *greater* than the colonization rate. If it is not a typo, this statement certainly deserves additional support or explanation.

2. Page 81. Reword sentence. There is no question that ocean conditions play an important role in determining abundance and productivity, but that differs from “attributing the decline largely to changes in ocean conditions.” Lawson, in particular, highlights the interplay between freshwater habitat conditions and ocean cycles, as illustrated in Figure 23.
3. Page 83. Although salmon are adapted to a variable environment, there are limits to their plasticity. Given that coho salmon in California are at the southern end of their range, it is reasonable to expect that for things like temperature tolerances and minimum flows, they may have little capacity to handle variation that falls outside of the natural range.
4. Page 83. Understanding of mortality caused by pathogens in the wild is poor, as it is difficult to determine the proximate and ultimate causes of death in the wild (i.e., when fish weakened by disease are consumed by predators before they die). Currently, there is insufficient data from which to draw meaningful conclusions about the importance of disease in regulating populations in the wild.
5. Page 88. Overall, this section does a good job of covering hatchery related issues. It seems, however, somewhat incongruous that the opening paragraphs highlight the

potential “positive” aspects of hatcheries when the majority of the text in the section is about adverse consequences. (In fact, since the purpose of the chapter is to identify causes of declines in salmon, then the statements regarding supposed hatchery benefits are would be most appropriate in Chapter VII). At the very least, it would seem appropriate for the content of the introduction to parallel the content of the body of the section (i.e., raise the many issues associated with hatcheries, rather than focusing on potential—and highly uncertain—benefits of hatcheries for conservation). Furthermore, potential uses of hatcheries in conservation are more effectively discussed after the reader understands the major genetic and ecological issues associated with hatcheries.

6. Page 95. Hatchery introgression is cited as a possible cause of “low levels of distinctiveness found among California coho populations”, but the data from the Bartley et al. study found a relatively high level of distinctiveness between these populations. To what extent this effect is due to the small data set is not known, but this statement is not justified.
7. Page 96. Footnote #15. The first sentence is incorrect. It should read “The probability that an individual has different alleles on the maternal and paternal chromosome.”
- 7a. It is probably worthwhile to draw the distinction between *total* genetic diversity and *adaptive* genetic diversity. Unique but maladaptive genes, while they contribute to overall genetic diversity, actually reduce the ability of population to respond to change.
8. Page 99. Footnote #18. This footnote should occur earlier. It is a definition for a term already in use in the text.
9. Page 100. Instead of arbitrarily choosing the high  $N_e/N$  ratio (0.33), to be conservative, you should really assume that the lower (0.1) estimate holds when estimating necessary population sizes, given all the assumptions that go into these numbers. This would result in targets that were higher.
10. Page 105. Unclear whether temperatures mentioned are average daily values, maximum summer values, etc. Also, should note that changes in diel variation are potentially as important as changes in summer maxima or average temperatures.
11. Page 108. Should note the distinction between annual water yield and peak flows. With the latter, routing of water more quickly to the stream channel likely has the greatest effect on peak flows (rather than reduced evapotranspiration).
12. Page 112. Somewhere in this section, effects on temperatures below dams (which depend on whether releases are hypolimnetic or epilimnetic) should be mentioned.
13. Overall, this chapter provides a very thorough accounting of various human activities that influence salmonids and their habitats. The major issues are identified and given treatment at an appropriate level of depth for this document. One area in which the chapter could be improved is through more consistent referencing of the primary literature. Some sections provide substantial documentation in support of various arguments. Other sections have few if any citations. We have marked a few of the more notable places where additional citations are needed, but the entire chapter should be reviewed to ensure that assertions are adequately supported.

#### Chapter VIII.

1. This section generally looks good. Only the comments regarding trends in SONC coho salmon need to be addressed.

#### Chapter IX.

1. Recommendations to list the SONC coho salmon ESU as threatened and the CCC ESU as endangered are consistent with conclusions of NMFS SWFSC scientists (NMFS 2001). Some discussion of whether stocks south of San Francisco Bay will continue to be treated separately versus lumped with the remainder of the CCC ESU is needed.

#### Chapter X.

1. Again, with CDFG acknowledging that metapopulation considerations are important, the omission of any mention of stocks south of San Francisco Bay is noteworthy.
2. TRTs do not develop recovery plans...their function is to develop the biological delisting criteria for each ESU within the domain. An implementation team will work with other agencies to develop the actual recovery plans.

#### Additional references to consider:

Cole, James. 2000. Coastal Sea Surface Temperature and Coho Salmon Production off the Northwest United States. Fisheries Oceanography. 9:1\_16.

Currens, K. 1997. Evolution and risk in conservation of Pacific salmon. Ph.D. thesis, Oregon State University, Corvallis, OR.

Hare, S.R., Mantua, N.J., and Francis, R.C. 1999. Inverse production regimes: Alaska and West Coast Pacific salmon. Fisheries 24: 6-14.

Hilborn, R. and C. Coronado. 1997. Changes in ocean survival of coho and chinook salmon in the Pacific Northwest. pp. 9\_18 in: Emmett, R.L. and M. H. Schiewe. (eds). Estuarine and Ocean Survival of Northeastern Pacific salmon: Proceedings of the workshop. NOAA Tech. Memo. NMFS\_NWFSC\_29, 313 p.

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**William Trush, Ph.D., Director, Institute of River Ecosystems; Adjunct Professor, Dept. of Fisheries, Humboldt State University**

Date: April 17, 2002

To: Larry Week, Chief Native Anadromous Fish and Watershed Branch

From: Bill Trush, Adjunct Professor in Humboldt State University Fisheries Department

Re: Comments, as a peer reviewer, regarding the April 2002 draft of *Status Review of California Coho Salmon*

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Thank-you for the opportunity to review CDFG's *Status Review of California Coho Salmon Draft* April 2002 ("Status Review"). I am not an expert on coho genetics. Rather my expertise is in assessing potential cumulative impacts to anadromous salmonids.

After reading the Executive Summary and being a self-confessed data junkie, I turned straight to the appendices (F2) to determine how well CDFG's assessment fared in portions of the Northcoast most familiar to me. I live on Lindsay Creek, a tributary to the Mad River. The Status Review comments on Lindsay Creek were detailed (p.44) given the broad scope of the Status Review, even noting the absence of coho on Grassy Creek (which historically must have supported coho). In the upper South Fork Eel River, the results from tributaries surveyed for coho presence/absence seemed accurate. I'm not sure how streams such as Fox Creek and Elder Creek are integrated into the assessment given these were likely never coho streams (though I have seen a few adults in the first 300 m of lower Elder Creek). Were these streams excluded from the presence/absence analysis? In contrast, lower Rock Creek is listed as having no coho observed. Though I have seen a few adults and juveniles in the 1980's, the lower 1.2 km has many geomorphic features favoring coho habitat, especially wide meanders through a low Eel River terrace. Rock Creek most likely received considerable historic coho use, and therefore should be included in a presence/absence analysis. To satisfy my scientific curiosity I could use more detail in the Status Review on the presence/absence analysis, though this level of detail may not be practically suited for the present draft.

The background life history section was complete. Drawing boundaries on the landscape is always difficult and somewhat arbitrary. The Mattole River population is so low that endangered status would be warranted, even though the Mattole watershed is just outside the CCC Coho ESU.

Even though suspended sediment and turbidity are discussed (p.27), the matrix of "fundamental habitat elements and suitable ranges for coho salmon life stages" (Table 4 on pp.32-33) omits the effects of turbidity on juvenile life stages and uses "ounces/gal" of sediment that affects adult coho. Given that the best physical variable for measuring cumulative watershed effects is suspended sediment, the matrix should devote considerable attention to this pivotal physical variable. There is a tremendous literature compiled by several investigators. A key to the recovery of coho in Northern California will be the formation and enforcement of credible suspended sediment and turbidity thresholds. This is a serious omission, and indicates that CDFG has not spent the necessary effort investigating meaningful quantitative criteria. I have often wondered why CDFG and the RWQCB have not produced a joint guideline for acceptable suspended sediment and turbidity thresholds, given their similar stewardship responsibilities for protecting fish and beneficial uses. Perhaps this would be a good recommendation for Chapter IX? Another recommendation, of using 30% to 50% of the annual flow for adult migration



(p.32), needs revision. Davenport Creek with a 0.93 mi<sup>2</sup> watershed behind my house had 17 redds constructed this past December and January. Riffle depths at 50% of the annual flow ranged from 2.5 to 3.4 inches deep, not the depths recommended for threatened fish passage! Other examples for poor quantitative measures can be taken from this matrix. CDFG should consider either dramatically improving it or eliminating it (perhaps making it a recommended task in Chapter IX).

I was asked as a reviewer: “Does the report seem reasonably complete and accurate with regard to its assessment of whether the continued existence of coho salmon north of San Francisco is in serious danger or is threatened by present or threatened modification or destruction of its habitat?” The Status Review does not present an accurate assessment of recent forestry activities (pp. 147 to 154) relative to present and potential future degradation of coho habitat. State and federal agencies, except staff of the North Coast Regional Quality Control Board, continue to discount obvious significant cumulative impacts directly attributable to excessive timber harvest rates in the SONCC Coho ESU. I was a member of the Scientific Review Panel (SRP) tasked by the California Resources Agency and NMFS with evaluating the effectiveness of the forest practice rules (FPRs) and their implementation in protecting Northern California salmon. In the *Report of the Scientific Review Panel of California Forest Practice Rules and Salmonid Habitat* (June 1999) the panel concludes that: “the FPRs, including their implementation (the “THP” process) do not ensure protection of anadromous salmon populations. The prime deficiency of the FPRs is the lack of a watershed analysis approach capable of assessing cumulative effects attributable to timber harvesting and other non-forestry activities on a watershed scale.” The panel also recommended that a range of maximum harvest rates be established in lieu of having a functional watershed analysis in place for all Northcoast watersheds. The Status Review leaves the impression that the SRP’s conclusion has been adequately addressed since 1999. The Status Review states (p.149), “Based upon the SRP’s findings and recommendations, the BOF adopted interim FPRs that went into effect in the summer of 2000.” Many good measures recommended by the SRP have been incorporated into new rules changes and procedures. But concluding as a scientist in 2002, and not as spokesperson for the now-extinct Science Review Panel, the measures adopted by the Board of Forestry (BOF) in 2000 and afterwards have been insufficient to alter the conclusion of the SRP’s report. For example, the NCWAP proposal does not remotely meet what the SRP Report outlines as necessary for a functional watershed analysis program that scientifically addresses potential cumulative watershed effects.

The status quo of chronic habitat degradation, and in many watersheds accelerated degradation, has not been abetted or reversed. Many key SRP recommendations were rejected, while other key recommendations were highly altered (e.g., recent BOF changes to determining the Watercourse Transition Line along unconfined Class I channels). The Pacific Lumber Company Habitat Conservation Plan (pp. 150 and 151), portrayed in the Status Review as a model for protecting habitat, is a prime example of not addressing cumulative impacts attributable to excessive harvest rates, highlighting what the SRP hoped to prevent. While many measures to reduce impacts cited on p.151 are needed, coho habitat cannot be sustained (let alone recovered) when harvest rates exceed 80% in tributary watersheds within 10 years. Recent (since the mid-1990s) excessive timber harvest rates in Freshwater Creek and Elk River have generated some of the muddiest streams in Northern California, creating turbidities that exceed background conditions in comparable second-growth watersheds by more than one thousand percent. Notwithstanding, CDF continues to approve THPs in these watersheds, justified by an antiquated doctrine that Best Management Practices (BMPs) prevent significant cumulative watershed impacts. The agencies, with the exception noted, are focusing on how to harvest and manage an acre of timberland while side-stepping the critical issue of how many acres can be safely harvested. Until cumulative impacts due to the rate of timber harvest are part of the decision-making process (and not the lip

service provided in the FPR), future prospects for healthy coho populations in many watersheds of the SONCC Coho ESU will be bleak.

CDFG in Chapter IX must recommend realistic management activities and inter-agency strategies (some painful) to aid coho recovery in order to justify threatened status in the SONCC Coho ESU. For example, how will CDFG help make CDF (p.192) “an active partner in stabilization and restoration of coho habitat within wildland areas though their authority for timberland management and wildland and rural fire control” when CDF has done such a poor job to date? Why hasn’t CDFG supported the Regional Water Quality Control Board mandate to prevent excessive harvest rates in critical coho watersheds? Can the extremely high turbidities generated in Freshwater Creek tributaries really be meeting CDFG’s expectations for recovery (p.191)? These and other critical uncertainties directly bear on the Status Review’s conclusions in the Executive Summary (p.2): “...the Department believes that coho populations in the California portion of this ESU [SONCC Coho ESU] will likely become endangered in the foreseeable future in the absence of the protection and management required by CESA.” If the recent BOF changes and development of Habitat Conservation Plans are considered by CDFG adequate to protect and manage as required by the CESA, then a threatened status for the SONCC Coho ESU is not enough because I do not share the same conclusion.

In summary, the Department has done a commendable job collecting and synthesizing the data to quantitatively, as best as possible, justify their rationale for listing coho salmon in both ESUs. It wasn’t an easy undertaking. However, the Status Review’s assessment of present and future protection of coho habitat is deficient. If the coho’s threatened status in the SONCC Coho ESU really hinges on whether coho are receiving, and will receive, adequate protection and management to prevent “foreseeable” endangerment, the Status Review does not make a sufficient argument. I must reserve my opinion of threatened status in the SONCC Coho ESU until the recommendations for future management (Chapter IX) are available.

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14 April 2002

Larry Week, Chief  
Native Anadromous Fish and Watershed Branch  
California Department of Fish and Game  
1807 13<sup>th</sup> St, Suite 104  
Sacramento CA 95814

Re: Status review of California coho

Dear Mr. Week:

Thank you for giving me the opportunity to review the draft of *Status review of California coho salmon (Oncorhynchus kisutch)*. I was very impressed by the document. It was thorough and provided much new information on status of the coho, confirming the results of past studies (including my own) that coho salmon are in danger of extinction in California, even in the more northern parts of the state. While the decline of coho salmon populations in California is the result of many interacting factors, the overwhelming cause is degradation of their freshwater environments by logging, urbanization, farming, and other human activities that reduce water quality and quantity and reduce habitat complexity. I think the document demonstrates that the downward trends are so dramatic and pervasive that coho salmon should be listed as endangered throughout California, not just in the southernmost part of their range. While I agree that the danger of extinction of many populations is not as immediate in the northern part of their range, an abrupt change in ocean conditions that reduces ocean survival rates could accelerate ongoing declines in an unpredictable fashion. Listing all populations of coho in California as endangered would send a clear signal about the State's concern for their survival and the need to reverse the trends in degradation statewide of our coastal streams.

Here are some minor comments on the report itself.

Pages 21-22. There should be reference to the studies of Jennifer Nielsen which show there are multiple ways juvenile coho use stream and estuary habitats in California (See 1992, Trans. Amer. Fish. Soc. 121 617-634.)

Pages 32-33. Table 4 is very hard to understand, especially the Suitable Range column for almost all

elements. In particular I have never seen turbidity and dissolved oxygen expressed as “ounces/gallon” in the recent literature.

Page 36. first paragraph. Hatcheries can be sinks for wild fish as spawning adults. Data presented elsewhere in reports shows that declines of coho often continue or even accelerate after hatcheries have been established, suggesting that the additional removal of wild spawners from the population has hurt more than helped.

Pages 86-87. The discussion on marine predation is very thorough and rightly concludes there is no evidence of predation having a negative impact on coho salmon. It might be worth mentioning that sea lions in estuaries prey heavily on Pacific lampreys, perhaps reducing predation rates on salmon.

Pages 102. I think it is worthwhile pointing out in the text that Monschke (1996) and Lisle and Napolitano (1998) are studies in California. When the final draft of this report is prepared, any study working with California coho should clearly be identified as such, to demonstrate that much of the information on which the report does come from local sources.

Page 109. Large woody debris and elsewhere. Nickelson et al. (1992, in bibliography) and (1992, Can J Fish Aquat Sci 49: 790-794) indicate that overwintering habitat may be more critical than summer habitat for survival of juvenile coho in southern Oregon. This should be emphasized more in this section of the report and the statement on p 22 stated more strongly.

Page 164. Suction Dredging. The rather lengthy sentence ending in ... (Mapstone 1995) seems to be referring to the **precautionary principle** which is being advocated by more innovative fisheries managers. Why not quote Dayton (1998) more directly and mention the possibility of using the principle widely in reference to management of coho salmon? Our failure to use the precautionary principle as a basis for management is at the root of many of our fisheries declines, including coho salmon. Timber harvest plans, for example, should be required to demonstrate they do no harm to coho, rather than having overworked CDFG biologists have to prove they harm coho.

Page 195. Final sentence. Would be more accurate to say “Eventually, extinction of coho salmon throughout California could result.”

These comments are obviously minor and I find the report to be overall of very high quality in summarizing and interpreting the existing literature. I hope that much of the report will be published as a CDFG *Fish Bulletin* in the near future, so it can be a useful ‘benchmark’ reference for fisheries biologists interested in coho salmon management for a long time to come and not lost as just another report.

Sincerely,

Peter B. Moyle  
Professor